The Academic Consequences of Teacher Strikes

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Abstract

Teacher strikes are increasingly common in the United States, but their effects on students are understudied. Using data from Illinois and temporal variation in school district exposure to strikes, I show that teacher strikes reduce achievement and enrollment rates. Math and ELA achievement fall by 0.074 standard deviations, with larger and more persistent effects for younger students, alongside enrollment declines concentrated in early grades. Effects do not scale with strike duration and I find no post-strike changes in class-sizes or teacher compensation, suggesting changes in the student population may be a key channel driving these changes.

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1 Introduction

Teacher strikes are a powerful bargaining tool for unions and have resurged in the United States in recent years. Hundreds of district and state-wide strikes took place in 2018 and 2019, with teachers advocating for higher wages and increased school funding, followed by conflicts over school openings and working conditions during and after the COVID-19 pandemic. Yet despite this long history, there remains limited credible evidence on how strikes affect students, both in terms of what they learn and where they choose to be enrolled. This paper studies the effects of teacher strikes on student achievement and family schooling decisions.

Using a panel of strikes and district-level test scores, enrollment, staffing, and finance data from Illinois, I leverage variation in district-level exposure to strikes in a difference-in-differences framework to study their effect on student achievement and enrollment. I document modest district-level declines in math and ELA achievement of 0.074 standard deviations, with larger effects when students are exposed to a strike in earlier grades. Cohorts who experience a strike in fourth grade exhibit declines of 0.158 standard deviations in math and ELA that persist throughout elementary school.

Strikes are also followed by sustained declines in district enrollment. In the five years after a strike, enrollment falls by 3 percent relative to comparison districts. These effects are larger for younger students, consistent with greater mobility among families whose children have weaker attachment to a particular district, while older students may be less likely to switch with only a few years remaining. These enrollment changes do not differ by race or income.

I investigate potential mechanisms underlying these results. If lost instructional time were a key channel, longer strikes would be expected to have larger effects. Instead, I find that duration has no effect on enrollment and little evidence that it consistently predicts achievement outcomes. I also do not observe positive post-strike effects, such as improve-

ments in instructional resources, reductions in teaching burden or student–teacher ratios, or increases in teacher compensation.

These findings have two important implications. First, strikes are not short, contained shocks. They can have lasting effects on achievement and enrollment, particularly for younger students. Second, the mechanism analysis rules out foregone instructional time or changes to instructional resources as primary channels by which strikes impact achievement. The most consistent remaining explanation is that compositional change, driven by family mobility, plays an important role in shaping achievement outcomes.

This paper advances the small literature on teacher strikes in several ways. First, I examine strikes in Illinois, where a high incidence of strikes generates substantial variation in district exposure within a common testing regime and institutional context, providing the statistical power needed to estimate strike effects while improving comparability across districts. Second, I assemble a 25-year panel of test score data, extending the time horizon relative to prior studies. This long panel allows me to estimate dynamic treatment effects, examine persistence well beyond the strike year, and study strikes occurring under different labor market conditions and bargaining environments. Third, this is the first paper to study the effects of strikes on student enrollment in the United States. I show that enrollment declines following strikes, particularly among younger students, generating compositional changes that may influence district-level achievement estimates. District-level analyses implicitly assume stable student populations; I show that strikes are followed by sustained enrollment changes that could affect the interpretation of estimated strike effects.

The literature on strikes and student achievement is limited, with many existing studies focused outside the United States. Using political divisions across French and Flemish communities in Belgium, Belot and Webbink (2010) find imprecisely estimated evidence that strikes reduce educational attainment and increase grade repetition. Baker (2013) shows that teacher strikes in Ontario lasting longer than ten days can reduce test scores at the late

primary level, with the largest impact being a .29 SD reduction in math. Using cross-cohort variation in strike prevalence in Argentina, Jaume and Willén (2019) find that greater strike exposure during primary school reduces educational attainment, labor earnings, and occupational skill levels. Similarly, Abadía Alvarado et al. (2021) find that students in Colombia exposed to more and longer strikes obtain lower scores in math and reading.

A limitation of this evidence from other countries is that students in these contexts miss substantially more school, often 40 to 80 days per year (Jaume and Willén (2019), Abadía Alvarado et al. (2021)), than is typical in the United States. As a result, it is unclear whether these findings generalize to a setting where strikes are less frequent and typically shorter in duration.

By contrast, there is little causal evidence on the effects of teacher strikes in the United States. Early studies found mixed results. Analyses from Pennsylvania (Zwerling, 2008) and Ohio (Thornicorft, 1994) find no impact or relationship between teacher strikes and student test scores. A recent study by Lyon et al. (2024) that examines not just strikes, but all coordinated labor actions that lead to at least one day of school closure (including walk-outs, or sick-outs) finds little evidence on student achievement. They do, however, find that strikes lasting more than ten days can decrease math achievement in the short-run, but they can not rule out that there are other factors correlated with strike length that drive these results.

Teacher strikes may also affect schooling decisions through their influence on student mobility, yet enrollment responses have received little attention. If strikes induce mobility, assumptions that students remain at the same school throughout their schooling may bias achievement results. No papers examine the effects of teacher strikes on enrollment in the United States. The few studies on this relationship focus on Argentina. Jaume and Willén (2019) find enrollment declines of 1.8 to 5.3% after scaling for the average level of strikes (57 days). Narodowski et al. (2016) examine strikes as a potential driver of private school

enrollment growth in Argentina, but find no significant correlation. Other papers, such as Abadía Alvarado et al. (2021), do not examine enrollment changes and assume students remain in the same school throughout. Lyon et al. (2024), focusing on the United States, include time-varying district-level enrollment as a control.

This paper also contributes to the literature on the impacts of school disruptions on enrollment in the United States. Several papers find that the COVID-19 pandemic induced an enrollment shift from public schools. State level studies show that there were substantial enrollment drops for younger grades in Washington, Michigan, and Virginia (Tuchman and Heyward (2021), Musaddiq et al. (2022), Schueler and Miller (2024)) and middle grades in Massachusetts (Francis and Goodman, 2025). Francis and Goodman (2025) and Musaddiq et al. (2022) also find that enrollment not only declined in public schools, but that the pandemic had a lasting positive impact on private and home school enrollment. Though there are obvious differences between teacher strikes and pandemic related disruptions to school systems, there may be shared characteristics of frustration by parents about closures or delays with returning to the classroom.

This paper contributes new causal evidence on the effects of teacher strikes in the United States, with implications for ongoing debates over collective bargaining, school closures, and labor relations in public education. By documenting both achievement and enrollment responses over a long time horizon, and by showing that compositional dynamics may shape district-level estimates, the findings offer a more complete picture of how strikes may affect students and the communities schools serve.

2 Data

2.1 Teacher Strikes

I construct a dataset of teacher strikes in Illinois from 1996 through 2019 using three sources. The Illinois Educational Labor Relations Board (ELRB) annual reports cover the majority of strikes between 2010 and 2019. I supplement these with the Bureau of Labor Statistics' (BLS) Work Stoppages program, which captures major work stoppages involving more than 1,000 workers since 1993, and the Federal Mediation and Conciliation Service (FMCS), which covers smaller stoppages from 1984 through 2020.

For each strike, I observe the school district, start and end dates, duration, and the number of workers involved. I exclude strikes in private or religious schools and work stoppages involving non-instructional staff. I assign each strike to the academic year in which it took place to capture effects on that year's standardized test scores.

Illinois is one of twelve states where teacher strikes are legal. Teachers there have regularly exercised this right. Between 1995–96 and 2018–19, Illinois had 102 strikes across 90 school districts[1], an average of 4.25 strikes per year, but some years had as many as 10 strikes. On average, these strikes lasted 13.5 days.

2.2 Test Scores

To measure student achievement, I use district- and grade-level standardized testing data in math and English language arts (ELA). The Illinois State Board of Education (ISBE) reports Performance Levels by district and grade dating back to the 1995–96 academic year. Grades 3, 5, and 8 have the longest testing history, starting in 1996 for grades 3 and 8 and in 1999 for grade 5. All grades are tested statewide after 2006[2].

[1] See Figure A-1 in the Online Appendix for a full time series of strikes in Illinois during this time period.
[2] See Table A-1 in the Online Appendix for details.

A key challenge is that Illinois changed its standardized exams and proficiency cutoffs multiple times during the sample period, limiting the comparability of raw Performance Levels over time. Illinois administered the IGAP through the late 1990s, the ISAT from 1999 to 2014 (with revised performance standards in 2013), the PARCC from 2015 to 2018, and the IAR beginning in 2019. These changes generated sharp mechanical breaks in measured proficiency[3].

For a comparable measure of achievement, I use scaled test scores from the Stanford Education Data Archive (SEDA), where Reardon et al. (2017) place state math and reading exams for grades 3–8 from 2009–2019 onto a common national scale using the NAEP. I use an expanded version developed by Biasi et al. (2025), who extend the Illinois panel back to 1996. These grade-level scaled scores constitute my primary measure of student achievement. As I find negligible differences between math and ELA scores, I average them for each district-year. For my main outcome, I construct a district-level achievement index by averaging third, fifth, and eighth grade scaled scores, weighting each grade equally.

2.3 School District Covariates

I obtain annual district data on student enrollment, teacher and staffing information, and finance measures from the National Center for Education Statistics' (NCES) Common Core of Data (CCD), which has annual information on student enrollment and characteristics, staff counts across roles, and fiscal data for all school districts.

The enrollment data include total district-level enrollment and breakdowns by grade, race, sex, English Language Learner (ELL) status, Free and Reduced Price Lunch (FRPL) status, and Individualized Education Program (IEP) status. I measure staffing using teacher Full-Time Equivalents (FTEs) and financial resources using total expenditures on instruction per student. I also construct a measure of average teacher compensation by summing

[3]For example, see the raw Grade 3 Proficiency Levels in Figure A-2 in the Online Appendix.

instructional salaries and benefits and dividing by teacher FTEs. All dollar measures are inflation-adjusted to 2018 dollars using the CPI.

My analysis sample is limited to public, non-charter school districts serving at least grades 3 through 8. I omit districts that report having zero schools, that operate for fewer than ten years, or have fewer than 100 students on average.

2.4 Sample

I merge the strike data, test scores, and district covariates to construct an annual panel of Illinois school districts from the 1995–96 through 2018–19 academic years. I exclude years during and after the COVID-19 pandemic due to major disruptions to both student achievement and the teacher labor environment, and the possibility that pandemic-related strikes were driven by fundamentally different mechanisms.

Over this period, there are 768 school districts that serve at least grades 3 through 8, have an average enrollment of at least 100 students, and are observed for at least ten years. Of these districts, 711 merged to the Biasi et al. (2025) test score data. Seventy-four of these districts experience at least one teacher strike. I further exclude 7 school districts that have more than one strike during this period[4] and 1 district in which the strike occurred after standardized testing had already taken place that year.

As described in the following section, I identify effects by comparing districts that experience a strike to districts that strike at a later date. The final analysis sample therefore consists of 65 districts that experience a strike during the sample period.

[4] I also run all analyses on the sample that includes these multiple striking districts, using their first strike as the date of treatment. The results are not substantially different.

3 Results

3.1 School District-Level Analysis

3.1.1 School District-Level Empirical Strategy

I estimate the effect of teacher strikes on achievement and enrollment using variation in the timing of district exposure to strikes within a doubly robust difference-in-differences (DiD) framework. The design compares districts before and after their first strike to districts that strike later. I treat strikes as a staggered-adoption intervention: districts enter the treated state when they first experience a strike and remain treated thereafter, capturing both the immediate disruption and longer-run changes to collective bargaining agreements and labor–management relationships that may persist after the strike ends.

Causal interpretation relies on two identifying assumptions. First, in the absence of a strike, outcomes in treated and comparison districts would have followed parallel trends. I assess plausibility using event-study specifications that trace out pre-treatment dynamics. While the absence of differential pre-trends is neither necessary nor sufficient for common counterfactual trends, it provides evidence on whether treated and control districts were evolving similarly prior to a strike (Kahn-Lang and Lang, 2020). Second, the timing of strikes must be quasi-random with respect to potential outcomes, conditional on fixed effects and the comparison group. A concern is that strikes could be triggered by deteriorating conditions; flat pre-strike trends suggest this is unlikely. Although I cannot fully rule out contemporaneous shocks, the staggered timing of strikes across districts reduces the likelihood that estimates are driven by a single common confounder.

Striking districts in Illinois differ systematically from districts that never strike: they are larger, serve higher shares of Black, ELL, and FRPL students, and have lower average test scores[5]. While district fixed effects absorb these time-invariant level differences, a concern is

[5]See Table A-2 in the Online Appendix for more details.

that these differences suggest districts that never strike may follow systematically different outcome trajectories than those that eventually do. To mitigate this concern, I restrict the comparison group to not-yet-treated districts rather than never-treated districts. This approach compares districts that strike earlier to those that strike later, strengthening the plausibility that test scores and enrollment would have evolved along similar trends absent a strike.

I implement the doubly robust DiD estimator of Callaway and Sant'Anna (2021), which combines outcome regression and inverse probability weighting to estimate group-time average treatment effects and is consistent if either the outcome model or the propensity score is correctly specified (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020). This approach also avoids the biases of two-way fixed-effects estimators in staggered-adoption settings with heterogeneous and dynamic treatment effects (Goodman-Bacon, 2021; Sun and Abraham, 2021). I aggregate group-time effects to recover overall average treatment effects and dynamic effects across event time.

I examine the effect of strikes at the district level by estimating the following event-study specification:

$$Y_{dt} = \gamma_d + \tau_t + \sum_{e=-4, e\neq -1}^{4} \beta_e YearsFromStrike_{dt}^{e} + \varepsilon_{dt} \tag{1}$$

where Y_{dt} are student achievement and enrollment outcomes. I examine average district achievement, constructed by averaging test scores across third, fifth, and eighth grades, as well as each grade separately, and enrollment for the whole district and separately for grades K–4 and 5–8. District fixed effects γ_d absorb time-invariant differences across districts, and year fixed effects τ_t capture common statewide shocks. $YearsFromStrike_{dt}^{e}$ is an indicator equal to one if district d is e years relative to its first strike in year t. The omitted category is the year immediately preceding the strike ($e = -1$). The coefficients of interest, β_e, trace out dynamics before and after strike exposure.

3.1.2 School District-Level Student Achievement Results

Figure 1 presents event-study estimates of the effect of a teacher strike on district-level math and ELA achievement, normalized to the year prior to the strike. I report the average treatment effects on the treated (ATT) for below the figure. Across specifications, the estimates indicate modest but persistent declines in achievement following strike exposure.

Looking first at Panel (a), which shows the effects on average district test scores, both subjects exhibit a slight negative post-strike trajectory. Math and ELA achievement decline by 0.074 standard deviations on average following a strike. The event studies show an immediate drop in the strike year followed by persistently lower achievement in subsequent years, suggesting that achievement does not fully recover in the years immediately following a strike.

Panels (b) through (d) show the grade-level event studies for third, fifth, and eighth grade achievement, the grades which have the longest availability of data. The grade-specific point estimates are suggestive of a pattern in which declines are larger for younger students: the ATT for 3rd grade math and ELA is -0.091 standard deviations, -0.096 for fifth grade, and -0.02 for eighth grade. However, I cannot reject that the effects are equal across grades. As such, while the pattern is consistent with younger students being more affected, we must be cautious about drawing this conclusion.

Two features of the district-level event studies are worth highlighting, as they motivate the cohort-level analysis in Section 3.2.2. First, the negative effects persist well beyond the strike year. In Panel (b), for example, third-grade math scores are depressed not only at $e = 0$, when the tested students directly experienced the strike, but also at $e = 1$ and $e = 2$, when the third graders being tested were in second and first grade, respectively, during the strike year. This suggests that even students who were in earlier, untested grades at the time of the strike carry achievement deficits into later years. While the point estimates at longer horizons become less precisely estimated, they do not clearly shrink in magnitude, making it

difficult to distinguish persistent effects from a loss of statistical power. Second, the effects vary with grade level. Eighth graders show little contemporaneous response to the strike, and in subsequent years, as younger cohorts age into eighth grade, the estimated effects remain modest. This pattern suggests that the district-level results reflect a combination of direct disruption and cohort composition. A more intuitive way to look at these effects would be to follow a cohort over time, as opposed to looking at the same grade-level test over time. In the cohort-level analysis in Section 3.2.2, I disentangle this more precisely.

3.1.3 School District-Level Enrollment Results

Figure 2 Panel (a) shows a sustained decline of 3% in K-8 student enrollment following a strike. Breaking this out by grade in Panels (b) and (c), the point estimates suggest somewhat larger declines for younger grades (K–4) than older grades (5–8), though the estimates are similar in magnitude and unlikely to be statistically distinguishable. One interpretation is that families of younger students face lower costs of switching districts, while older students are more established and may only have a few years left before high school. I find no meaningful enrollment trends by race, ELL, IEP, or FRPL status[6], suggesting that compositional changes following a strike are not driven by the exit of a particular demographic group. However, this does not rule out selection along unobservable characteristics, such as prior achievement or parental engagement, which could still contribute to measured achievement declines.

In an attempt to understand where these families may be moving their children, I also ran regressions using the log student enrollment of neighboring[7] non-striking school districts, charter schools, and private schools. Due to the limitations of using district- and school-level data, as opposed to administrative data which could be used to follow individual students,

[6]See Figure A-3 in the Online Appendix for the effect of teacher strikes on the Log enrollment of different student subgroups.

[7]I define a school district or school as "neighboring" if it shares at least one county with the striking district, based on the 2020 Census School District Reference Maps.

I do not find any conclusive evidence of where families may be moving. My results suggest that enrollment also declines in the neighboring nonstriking districts and charter schools, but to a far lesser degree, and that enrollment at neighboring private schools appears to be increasing[8].

These district-level estimates combine multiple student cohorts observed at different stages of exposure, making it difficult to distinguish persistent learning effects from cohort composition changes. The cohort-level analysis in Section 3.2 addresses this limitation by following the same group of students over time.

3.2 Cohort-Level Analysis

3.2.1 Cohort-Level Empirical Strategy

The cohort analysis isolates how a group of students evolves after strike exposure, allowing persistence and age-of-exposure effects to be examined more directly than in district-level analyses.

I transform the district-year panel into a district-cohort-grade panel. I define a cohort by the academic year in which students are enrolled in third grade. I then construct cohort-by-grade measures of achievement and enrollment by re-indexing district-level, grade-specific outcomes over time. For example, the 2000 cohort consists of students enrolled in third grade in 2000, observed in fourth grade in 2001, fifth grade in 2002, and through eighth grade in 2005. The cohort's third-grade test score is taken from the district's third-grade scores in 2000, its fourth-grade score from the district's fourth-grade scores in 2001, and analogously for higher grades. I construct cohort-level enrollment measures analogously using grade-specific district enrollment counts.

In my sample, I only include cohorts whose third-grade year falls between 1996 and 2014, thus retaining only those cohorts that can be followed from third through eighth grade.

[8] See Figure A-4 in the Online Appendix.

This approach implicitly assumes that students progress through grades on schedule, neither repeating nor skipping grades, and remain within the same district. These assumptions cannot be verified without individual-level data.

I exploit variation in the grade at which cohorts are exposed to a strike by defining four treatment scenarios: strikes occurring when a cohort is in fourth, fifth, sixth, or seventh grade. Restricting attention to these grades ensures that outcomes are observed at least once before and after exposure. For each scenario, I construct a separate analysis sample. Consider the fourth-grade case. A cohort is treated if its district strikes while the cohort is in fourth grade. Cohorts in districts that strike after they complete eighth grade serve as controls. I exclude cohorts exposed to a strike in any other elementary grade. For example, a 2000 cohort from district A is treated if the district has a strike in 2001. A cohort from district B is a control if its district strikes in 2006 or later, since this cohort completes eighth grade in 2005.

I then construct an event-time variable centered on the cohort's fourth-grade year and stack all eligible cohorts to form the fourth-grade strike sample. I repeat this procedure for strikes occurring in fifth, sixth, and seventh grade, yielding four grade-specific event-study datasets. Then, I use a similar event study model as in Equation (1), with some slight changes to account for differences in event time and lead and lag periods for each of the four treatment scenarios:

$$Y_{cgdt} = \kappa_c + \lambda_g + \gamma_d + \sum_{e=3, e\neq r}^{8} \beta_e YearsFromStrike^{e}_{cgdt} + \varepsilon_{cgdt} \tag{2}$$

where Y_{cgdt} is the test score or log enrollment of cohort c in district d in event-time g (the grade the cohort is in) and academic-year t. I include cohort fixed effects κ_c, event-time (grade) fixed effects λ_g, and district fixed effects γ_d. The omitted category r is the grade immediately preceding the strike. The number of leads and lags varies depending on the grade of exposure. For the fourth-grade scenario, there is one pre-period (third grade, $r = 3$)

and five post-periods (fourth through eighth grade). For the fifth-grade scenario, there are two pre-periods ($r = 4$) and four post-periods. The pattern extends analogously for sixth and seventh grade.

3.2.2 Cohort-Level Student Achievement Results

Figure 3 reports event-study estimates normalized to the grade prior to strike exposure, along with ATTs for each case.

Panel (a) shows results for cohorts exposed to a strike in fourth grade. These cohorts experience large and immediate declines in achievement. In the year of the strike, math and ELA scores fell and remain negative in subsequent grades. The overall ATT indicates average declines of 0.158 standard deviations. These declines may reflect direct learning loss, but could also partly reflect changes in the composition of students remaining in the district, as documented in Section 3.2.3. Panels (b) through (d) show the effects on cohorts exposed in later grades. For exposure in fifth, sixth or seventh grade, the estimates of a strike on achievement are small and generally indistinguishable from zero.

Taken together, these results point to an age gradient in strike impacts. Cohorts exposed in earlier elementary grades exhibit larger and more persistent achievement declines than those exposed in later grades. This pattern is consistent with several explanations, including younger students being more reliant on structured instruction, but also with greater compositional turnover among younger cohorts, as I document in Section 3.2.3. Without individual-level data, I cannot distinguish between these channels. I also examine heterogeneity in the effects of a teacher strike on student achievement, but find no consistent evidence of effects differing by race or income[9].

[9] I examine heterogeneity using two additional test score sources: ISBE proficiency levels (2000–2012) by race, income, and grade, and SEDA scaled scores (Reardon et al., 2017) (2009–2019) by race and grade. Table A-3 in the Online Appendix shows suggestive evidence that effects may be larger for Black students using ISBE proficiency levels, but these results are not robust across test score measures or sample periods. Table A-4 similarly shows no statistically significant differences by income. Given the sensitivity to data source and time period, I do not draw definitive conclusions about heterogeneous effects by race or income.

3.2.3 Cohort-Level Enrollment Results

The cohort-level enrollment results in Figure 4 complement the district-level findings from Section 3.1.3 and shed light on how individual cohorts experience enrollment changes as they progress through elementary school.

Panel (a) shows that cohorts exposed to a strike in fourth grade experience the largest decline in class size. Enrollment drops sharply in the strike year and remains depressed through eighth grade, with an ATT of -0.026 log points. Panel (b) shows a similar, though slightly smaller, pattern for cohorts exposed in fifth grade (-0.022). By contrast, Panels (c) and (d) indicate that cohorts exposed in sixth or seventh grade show little change in enrollment, very few classmates leave the district in these later years.

Several factors may explain why enrollment responses are concentrated among younger cohorts. Families of younger students face lower switching costs: their children have fewer established peer relationships and extracurricular commitments, and parents have a longer horizon over which the benefits of switching can accrue. Older students may be only a year or two from high school, making a mid-elementary switch less appealing. Families of younger children not yet enrolled may also choose a different district after observing a strike, contributing to enrollment declines without any student actually leaving.

This age gradient in enrollment closely mirrors the age gradient in achievement, raising an important interpretive question: to what extent do the achievement declines reflect direct learning loss versus compositional change? If higher-achieving or more engaged families are disproportionately likely to leave after a strike, the observed decline in test scores could partly reflect who remains rather than what students have learned. Without individual-level data, I cannot fully decompose these channels, but the enrollment findings suggest that compositional change is a plausible contributor.

These enrollment dynamics motivate the analysis in Section 4, where I examine whether instructional time loss, changes in district resources, or teacher workforce adjustments can

account for the observed effects, or whether the compositional channel remains the most consistent explanation.

4 Potential Mechanisms

The preceding results show that teacher strikes are associated with persistent declines in achievement and enrollment concentrated among younger cohorts. I now explore several channels through which strikes may affect student outcomes: instructional disruption, changes in the teacher workforce, and changes in district resources. As I discuss below, none of these channels consistently explains the observed patterns, which points to compositional responses, documented in the enrollment results above, as a potentially central mechanism.

4.1 Lost Instructional Time

The most direct channel through which strikes could reduce achievement is the loss of instructional time. If this were the primary mechanism, longer strikes should generate larger achievement losses. Similarly, if parents respond to strikes by leaving the district, and if this response is driven by frustration that accumulates over the course of a work stoppage, longer strikes should also produce larger enrollment declines.

For my sample of 65 strikes, the average duration was 13.2 days, ranging from a single day to 123 days[10]. I test whether achievement losses scale with duration by estimating Equation (1) separately for strikes of different lengths and find no consistent relationship. The results are highly sensitive to the cutoff for defining "long" versus "short" strikes. Table A-5 Panel A in the Online Appendix shows that longer strikes appear more harmful when defined as exceeding the mean (13 days), but shorter strikes have the larger effect when the cutoff is the median (9 days). This non-monotonic relationship undermines the case that foregone

[10]See Figure A-5 in the Online Appendix for a histogram of the count of strikes by duration in days.

instructional days are the primary mechanism.

Illinois state statute requires school districts to provide a minimum of 176 days of student instruction. When a strike causes a district to fall below this threshold, districts may extend the school year, convert holidays or school improvement days into instructional days, or shorten winter or spring breaks. Districts may also choose not to make up lost days, though they risk state penalties for doing so. Because I restrict my analysis to strikes that occur before the administration of the standardized test, any make-up days occurring after the test would not affect measured achievement in the strike year. However, make-up days scheduled before testing or in subsequent years could attenuate the estimated effects, particularly for longer strikes, if districts are able to recover lost instructional content. Without data on whether and when make-up days were scheduled, I cannot directly test this possibility.

The enrollment results provide even clearer evidence against the instructional time channel. Table A-5 Panel B shows that enrollment declines are remarkably stable across all definitions of "long" and "short" strikes, falling by approximately 3% regardless of duration. This suggests that the occurrence of a strike matters more than its length for families' decisions, consistent with strikes eroding trust or signaling institutional dysfunction rather than a cumulative frustration response proportional to days missed.

4.2 Teacher Workforce and District Resources

I next examine whether strikes lead to changes in teaching conditions that could offset the negative effects on achievement. If strikes reduce class sizes or increase district resources and teacher compensation, these positive changes could partially counteract the disruption.

Figure 5 Panel (a) also shows that, following a strike, teacher full-time equivalents decline persistently, roughly in proportion to falling enrollment, leaving student–teacher ratios largely unchanged, as seen in Panel (b). Rather than seeing reductions to class sizes, teachers in striking districts face roughly the same student-to-teacher ratios as before the strike.

This null result rules out reduced teaching burden as a plausible offsetting channel.

The proportional decline in teacher FTEs also raises a question about the teacher workforce more broadly. Are districts actively reducing staff in response to lower enrollment, or are teachers leaving voluntarily in the aftermath of a contentious labor action? While the data do not allow me to distinguish between these possibilities, teacher attrition could itself be a mechanism affecting student outcomes if departing teachers are systematically different in quality or experience from those who remain.

I also find no changes in district resources or teacher compensation. Figure 5 Panels (c) and (d) show that instructional expenditures per student and average teacher compensation remain unchanged after a strike. These results provide little evidence that changes in district finances or pay could be offsetting the observed achievement losses. This is consistent with Lyon et al. (2024), who find that teacher strikes are only effective at increasing compensation in states where striking is prohibited by law, possibly reflecting selection in unions' willingness to strike.

5 Discussion

This paper studies the effects of teacher strikes on student achievement and enrollment using three decades of strike activity in Illinois and a staggered-adoption difference-in-differences design. Leveraging both district-level and cohort-based variation, I find that teacher strikes modestly reduce student achievement, by 0.05–0.10 standard deviations at the district level, and are followed by sustained enrollment declines of approximately 3.2%, with effects concentrated among students who are younger when they experience a strike and persisting well beyond the year of the strike.

I find little evidence that the observed achievement losses are driven primarily by foregone instructional time, changes in district resources, or changes in teacher compensation. Instead,

the observed enrollment declines, which do not scale with strike duration and do not differ by observable student demographics, may point to compositional responses as a key channel. Following a strike, districts experience sustained enrollment declines without large shifts in observable demographics. Nevertheless, if the families who leave (or choose not to enter) striking districts differ from those who remain along unobservable dimensions, such as prior achievement, parental engagement, or socioeconomic resources not captured by FRPL status, then the measured decline in test scores may partly reflect compositional change rather than learning loss alone. These findings suggest that the consequences of teacher strikes extend beyond short-run instructional disruptions, triggering longer-run adjustments in who remains in a district and who leaves.

This study has several limitations. The analysis uses aggregated achievement measures and cannot directly observe individual student mobility or learning trajectories. Future research using linked student data could more precisely distinguish learning loss from compositional change, a distinction central to the interpretation of my results. In addition, this study focuses on public school districts in a single state where strikes are legal. In states where strikes are prohibited, work stoppages may take different forms (such as sick-outs or walk-outs), may carry different political salience, and may elicit different enrollment responses. The magnitudes documented here may not directly generalize to other institutional settings, though the broader finding that strikes affect not just achievement but also enrollment composition is likely relevant across contexts.

This paper contributes to a growing literature on teacher labor markets by showing that strikes are not neutral shocks. They have persistent and unequal effects on students, shaped not only by disruptions to instruction but also by how families respond. Understanding these dynamics is critical for designing labor relations institutions that protect teachers' bargaining power while minimizing unintended consequences for the students that schools are meant to serve.

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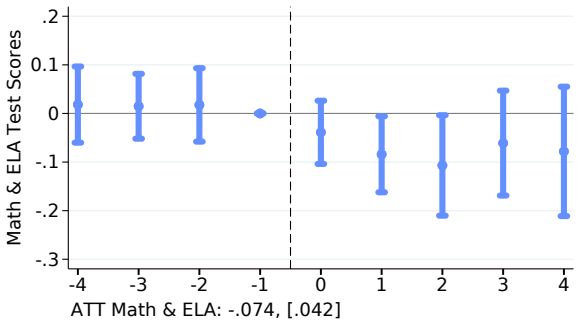
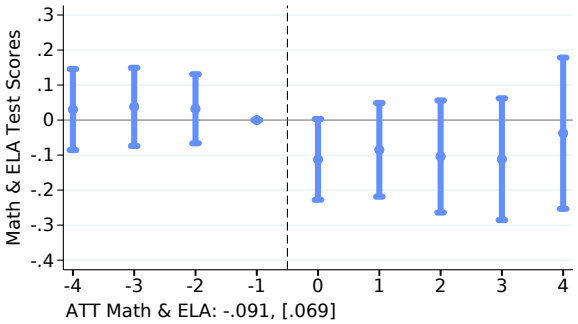
6 Exhibits

Figure 1: Effect of a Strike on Achievement at the School District-Level

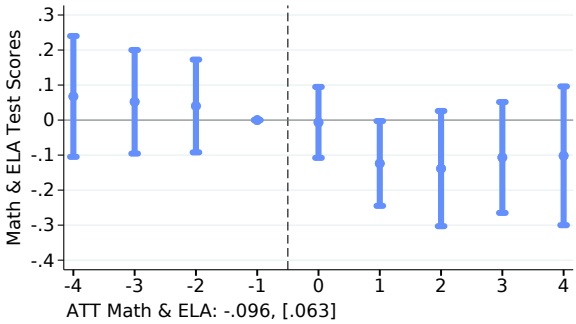
(a) District Average Math & ELA Test Scores



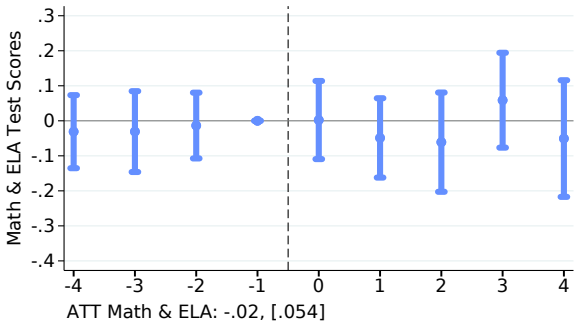
(b) 3rd Grade Avg Math & ELA Test Scores



(c) 5th Grade Avg Math & ELA Test Scores



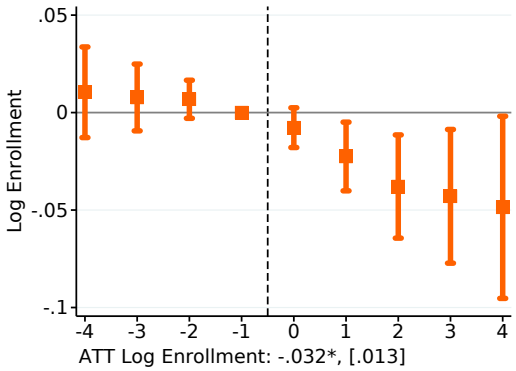
(d) 8th Grade Avg Math & ELA Test Scores



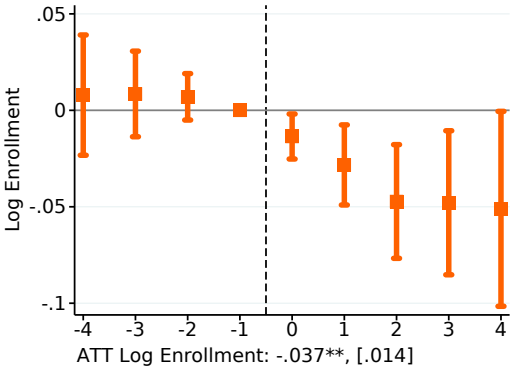
Note: Each panel reports event-study estimates from Equation (1) using the Callaway and Sant'Anna (2021) doubly robust estimator with not-yet-treated districts as controls. Coefficients are normalized to the year prior to the strike ($e = -1$). Panel (a) uses the district average of third-, fifth-, and eighth-grade Biasi et al. (2025) scaled math and ELA test scores. Panels (b)–(d) report grade-specific estimates of average math and ELA scaled test scores. The average treatment effect on the treated (ATT) and its standard error are reported below each panel. Vertical bars indicate 95% confidence intervals.

Figure 2: Effect of a Strike on School District Enrollment

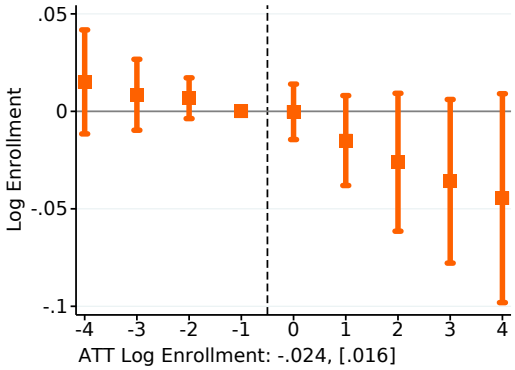
(a) Ln K-8 Enrollment in District



(b) Ln K-4 Enrollment in District

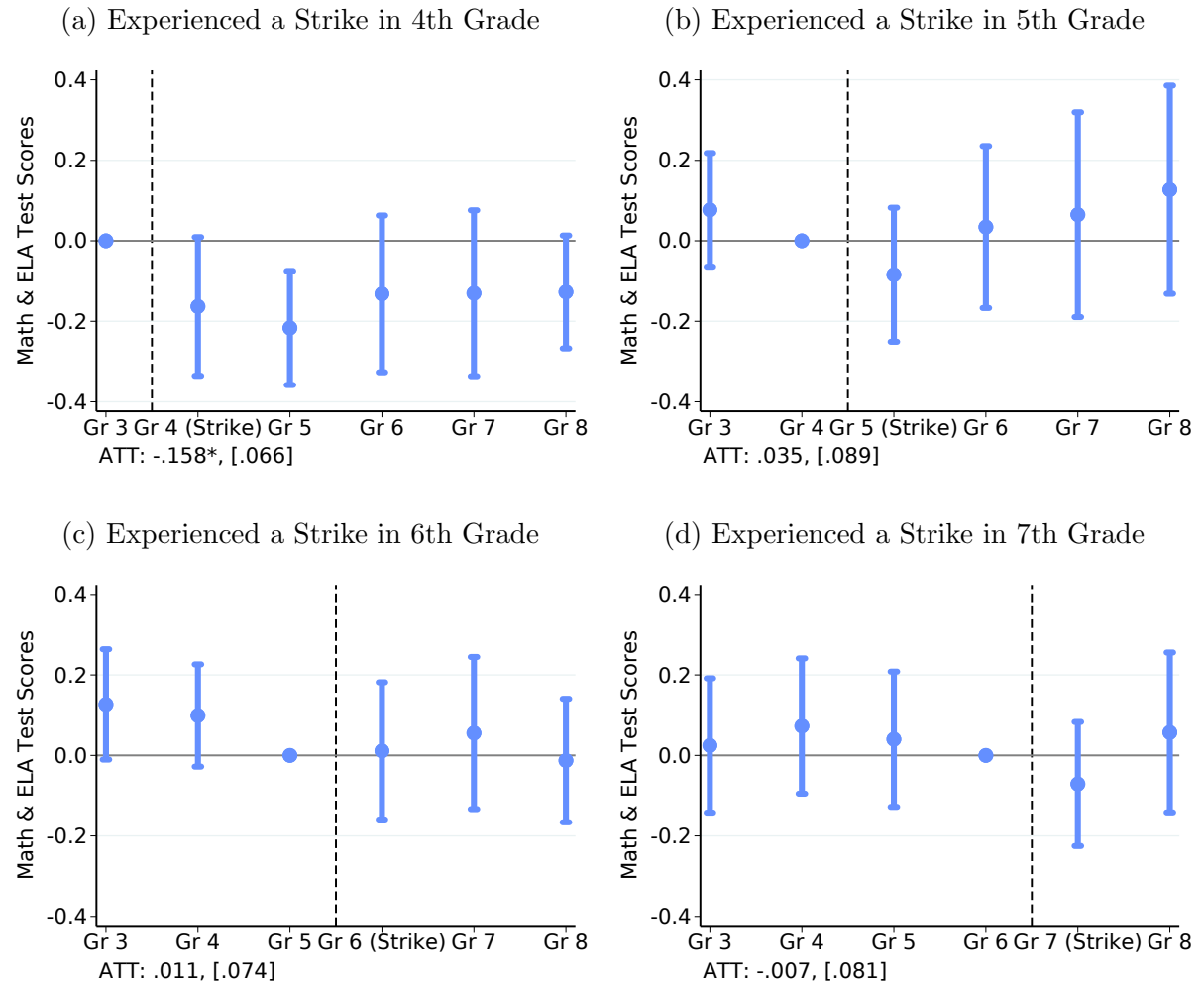


(c) Ln 5-8 Enrollment in District



Note: Each panel reports event-study estimates from Equation (1) using the Callaway and Sant'Anna (2021) doubly robust estimator with not-yet-treated districts as controls. Coefficients are normalized to the year prior to the strike ($e = -1$). The outcome is log enrollment for the indicated grade range. The ATT and its standard error are reported below each panel. Vertical bars indicate 95% confidence intervals.

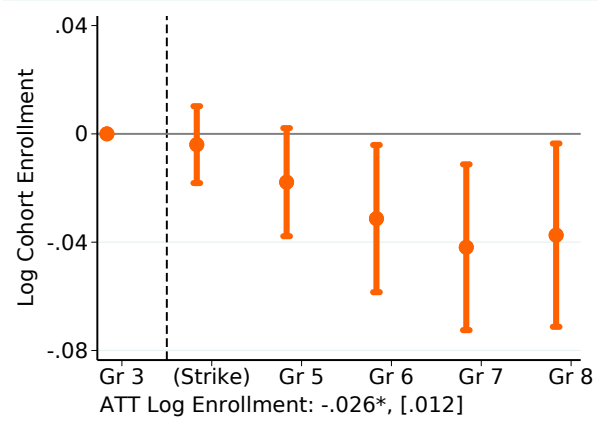
Figure 3: Effect of a Strike on Achievement at the Cohort-Level



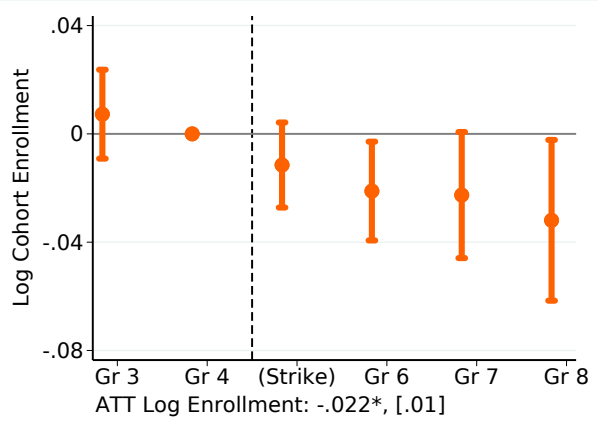
Note: Each panel reports event-study estimates from Equation (2) for cohorts exposed to a strike in the indicated grade. Coefficients are normalized to the grade immediately preceding the strike. Math estimates are shown in blue (circles) and ELA in orange (squares). Controls include cohort, district, and event-time fixed effects, with not-yet-treated cohorts as controls. The ATT and its standard error for each subject are reported below each panel. Vertical bars indicate 95% confidence intervals.

Figure 4: Effect of a Strike on Enrollment at the Cohort-Level

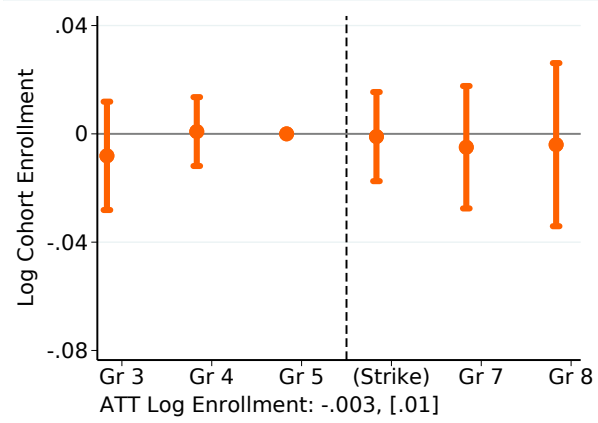
(a) Experienced a Strike in 4th Grade



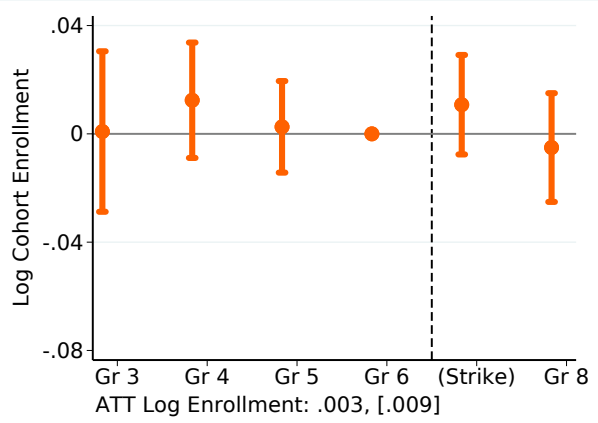
(b) Experienced a Strike in 5th Grade



(c) Experienced a Strike in 6th Grade



(d) Experienced a Strike in 7th Grade

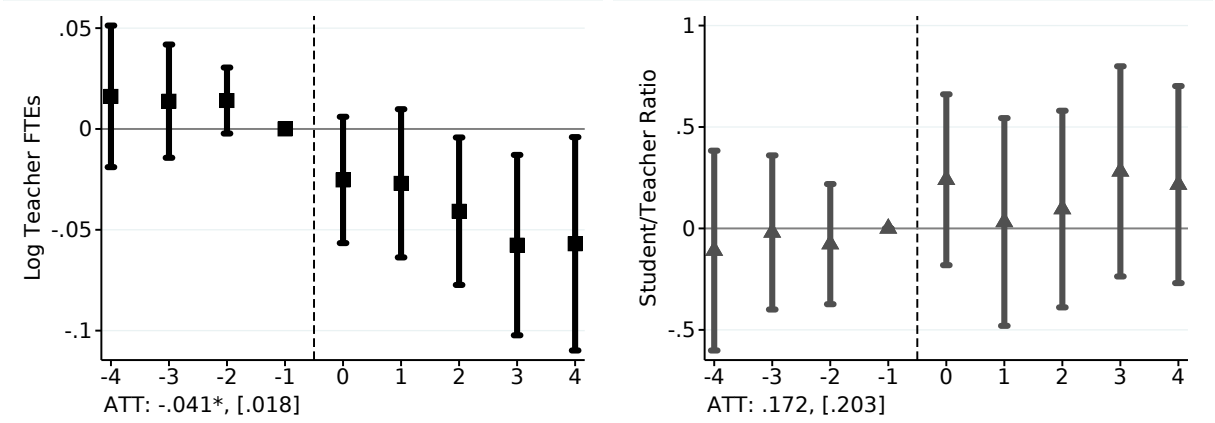


Note: Each panel reports event-study estimates from Equation (2) for cohorts exposed to a strike in the indicated grade. The outcome is log grade-level enrollment. Coefficients are normalized to the grade immediately preceding the strike. Controls include cohort, district, and event-time fixed effects, with not-yet-treated cohorts as controls. The ATT and its standard error are reported below each panel. Vertical bars indicate 95% confidence intervals.

Figure 5: Effect of a Strike on School District-Level Characteristics

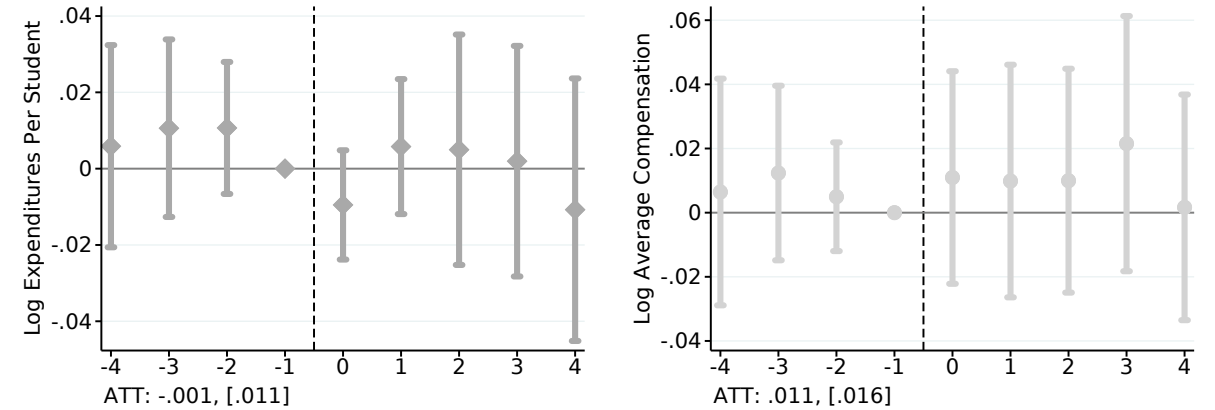
(a) Log Teacher FTEs

(b) Student/Teacher Ratio



(c) Log Instructional Expenditures Per Student

(d) Log Average Teacher Compensation



Note: Each panel reports event-study estimates from Equation (1) using the Callaway and Sant'Anna (2021) doubly robust estimator with not-yet-treated districts as controls. Coefficients are normalized to the year prior to the strike ($e = -1$). Log average teacher compensation is defined as the sum of instructional salaries and instructional benefits divided by teacher FTEs. All dollar amounts are inflation-adjusted to 2018 dollars using the CPI. The ATT and its standard error are reported below each panel. Vertical bars indicate 95% confidence intervals.